SCIENCE.

FRIDAY, DECEMBER 5, 1884.

COMMENT AND CRITICISM.

SEVERAL QUESTIONS of importance, affecting the scientific work of the government, will come before congress during the session just opened. First among these will be the organization of the two great surveys and of the signal-service. Our readers have already been informed through this journal, as well as through the newspapers, that the question of the management of these bureaus was referred to a congressional commission, composed of three senators and three representatives, who are required by law to report their conclusions, by bill or otherwise, on or before the third Monday in December. This commission invoked the aid of the National academy of sciences, and a report from a committee of this body is already in the hands of the commission. The conclusions of this report have not been authoritatively made public; but, according to a newspaper account, it recommends nothing more radical than the concentration of the bureaus in question under a single department of the government, and the appointment of a commission to control the policy both of the coast and geological surveys.

It was naturally expected that the commission would itself enter upon a thorough and minute investigation of the subject, — a view which was strengthened by the fact that a meeting was called for Nov. 11; but, up to the present time, there are no indications that the commission is going to enter upon any very serious labors. Only one week will remain to it when these lines reach our readers, and we have not been able to learn that it has done any thing but postpone its meetings. In this it only reflects the natural tendency of the congress whose term is about to expire. A short session is, under any circumstances, unfavorable for new legislation, and the house would naturally be inclined to await the views of the incoming administration before adopting any measures which might hamper it. We must also remember that it is much easier to stop a bill than to pass it, and that we can hardly expect a measure to be devised which will command the unanimous approval of all concerned. The establishment of a bureau of electrical standards, as proposed by the electrical congress at Philadelphia, must take its chances with the measures for re-organization of the surveys. There is no likelihood of an independent measure for such a bureau being successful.

Other matters which may be expected to arise are international in character; namely, the legalization of the conclusions of the Paris electrical conference and of our own meridian conference. In both these matters we can only hope that congress will make haste very slowly. There is no apparent pressing reason for speedy action on either subject, since both might very well take care of themselves without legislation; and there is a chance of much harm being done by too hastily adopting conclusions which may soon be found to need revision. The standard of light of the Paris conference has not been shown to be realizable in practice, and the accuracy of its ohm is already being called in question. In the case of the meridian conference, so far as its conclusions define the counting of longitudes from Greenwich, they merely authorize our universal practice, and there is hardly more need of our legislating upon the subject than there is of enacting that people shall eat their dinners. If its universal day is found convenient, it will come into use of itself; if not, congress ought not to legalize it. Altogether, we do not see much prospect of very good measures being devised between now and the 4th of March; and we may as well, therefore, reconcile our-

selves to the prospect of nothing being done beyond the passage of the regular appropriation bills.

The rapid increase of an organic species in a new and favorable habitat has been illustrated by Darwin's description of the cardoon, that so quickly spread over the Argentine pampas. A more recent example of similar success on the part of a colonizer is seen in the English sparrows that have become so numerous around our eastern cities; but the most peculiar illustration of the effect of better opportunity on an old form is found in the rapid development of seismometric instruments in Japan. The instrumental observation of earthquakes has had but moderate advance in Europe of late years: earthquakes are too rare there to give the study sufficient nourishment for development much beyond its present stand. But the English and German professors imported a few years ago, by the Japanese government, to build up the University at Tokio, found the numerous light shocks in that country to be just the stimulant needed for the rapid multiplication of seismometers; and as a result the European stock planted there has sprung up in such number, variety, and perfection, as to leave its relatives elsewhere in the world far behind.

We dwellers in a land of relatively few earthquakes may profit by the studies made in Japan, as reviewed in another column, instead of waiting for the slow development of seismometry among ourselves. Even a brief examination of Professor Ewing's memoir and of the transactions of the Seismological society of Japan will show how many of Mallet's theorems need revision in the light of these newer and more practical studies, and how great is the need of systematic and co-ordinated observation in the search for the seat and cause of seismic disturbances. The study is regenerated since Mallet's time. The newly opened opportunity for its cultivation in this country is described in our notes.

At the foot of the titlepage of Professor Ewing's recent work on earthquakes, printed by the Japanese government, the date is given as '2543 (Japanese era). 1883 A.D.' In a matter of chronology, where, to avoid confusion in dates, a uniform system among all nations is the great desideratum, it would seem almost superfluous to suggest the advisability of dropping from the works which the Japanese publish in foreign languages the use of an era which has never been employed in either business or official correspondence or records by the Japanese themselves, which was invented and officially adopted only twelve years ago, and which, though claiming to reckon from the time of the accession to the throne of the first Japanese emperor, has no reliable historical basis whatever, for at least the first twelve or thirteen hundred years, perhaps more, of its claimed antiquity.

In a notice of the first annual report of the New-York experiment-station (*Science*, vol. ii. No. 42) we took occasion to point out what appeared to us to be the mistaken view of its director regarding the duties of an experiment-station. It would seem either that our apprehension of his meaning in the preliminary passages of that report was imperfect, or that time and further experience have led to a revision of opinion upon the point in question. On p. 22 of the present report we find the following: "Before much real practical advance can be made in bringing agricultural pursuits within the domain of applied science, much work of a purely scientific character must be accomplished; and unpopular as it may be for the worker, yet that worker who investigates agricultural problems, not from the economic but from the reason stand-point, is doing the best work, and the work which in the end will be found most profitable in its applications." We quote this paragraph because it so well expresses the opinion which we urged in our review of the first report, that an experiment-station is primarily a scientific institution, intended to promote the advancement of the science of agriculture, and capable of the high-

est and most permanent usefulness, only when it fulfils this intention as far as possible.

Whether the words we have quoted, and others of a similar tenor, mark a change of opinion on the part of the director of the New-York station, or are only a clearer expression of convictions previously held, we do not undertake to say. In either case, we are glad to see the weight of this important institution cast in favor of the scientific conception of an experiment-station. The great need of agriculture to-day is not new varieties of plants, or improved breeds of animals; new methods of cultivating the soil, or improved systems of farming. All these, and many other like things, are good; but the two great wants are a better knowledge of principles, and greater intelligence to apply them. For the latter we must look to our agricultural schools: the former we should require from our experiment-stations.

We do not hold that an experiment-station should never undertake to originate or test new varieties of plants and animals or new agricultural methods,—often work of this general character will be demanded of it by the public, and will prove of great public utility,—but, in our view, it should not be allowed to be, or to appear to be, the chief end of the station. The two kinds of work are both important, but we question the advisability of attempting to unite them in one institution and under one management. Each requires facilities and talents peculiar to itself; and it seems doubtful, whether, as a rule, one institution will be able to provide good facilities for both kinds of experimentation, and still more doubtful whether it can find combined in one person the diverse knowledge and training required for their successful prosecution. With the growth of agricultural experimentation there might profitably be, we suspect, in the majority of cases, a subdivision of it into two overlapping yet independent classes. We should have, first, the experiment-station proper, aiming chiefly at a further elucidation of the laws and principles underlying agriculture; and, second, the experimental farm, devoted mainly to carrying out upon a farming scale the principles worked out by the experiment-station.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Psychical research.

YOUR issue of Oct. 17 contained two articles which are of good omen for the future of 'psychical research' in America. Of the first, the editorial article, I need say little. It is cordially welcomed by my colleagues and myself for its recognition of the far-reaching importance of an enterprise in the further development of which our society will, we hope, go hand in hand with yours. With the second article, on 'psychic force,' our agreement is less complete; but we still find nothing to complain of in the general attitude of the distinguished writer. He, too, recognizes the legitimacy of the inquiry, while clearly apprehending its difficulties. He describes with entire justice the two opposed classes between which psychical research has to clear a path,—the party of easy credulity, and the party of easy incredulity; and he points out with no more than proper emphasis the rigorous caution which every forward step demands. Fraud and superstition have naturally seized on what science has so systematically neglected; and those who now endeavor to take the subject up from the scientific side must accept the fact and its consequences.

So far, then, we are wholly at one with Professor Newcomb; but we cannot quite so readily follow him in his criticisms of our own doings. He begins by condemning one of our public appeals for information; but his strictures seem to assume that all the information which the appeal brings in will be regarded by us as a safe basis for conclusions. The appeal is, of course, merely a first step, for which it would be difficult to imagine any effective substitute; though I may mention that a very large amount of our information comes to us through private channels. The sifting and treatment of the evidence according to scientific canons must be a subsequent labor, the *rationale* of which could not be set forth, or even suggested, in the terms of a short advertisement. And of this labor no portion is more important than the one which we are glad to find Professor Newcomb so explicitly recognizing,—the application of the doctrine of chances. In all those branches of our inquiry where questions of *coincidence* occur, it is clearly essential to ascertain, as definitely as may be, how far the coincidences may fairly be ascribed to *chance*. We have taken, and are still taking, great pains to obtain this definite information. Very wide inquiries have been made; and the results, though far from complete, may still, I think, claim decidedly more validity, as a basis of computation, than Professor Newcomb's guess at what "any physician will consider quite within the bounds of probability." It would require more space than I can ask for, to comment on Professor Newcomb's numerical argument in detail. But I may remark that he seems to confuse the argument by classing all together what he calls 'dreams, illusions, visions,' etc.; at least, if

he means to include in this heterogeneous group visual hallucinations of waking persons, which we regard as by far the most important phenomena from an evidential point of view. If any one, in his waking moments, experiences apparitions of human forms as often as once a week, which is the degree of frequency that Professor Newcomb's calculation assumes, it is obvious that the approximate coincidence of one of these apparitions with the death of the corresponding human being will be an insignificant accident. But we have not ourselves met with any specimen of this class. We have collected more than a hundred first-hand cases of apparitions closely coinciding with the time of death of the person seen; and it is only in a small minority of such cases that our informants, according to their own account, have had any other hallucination than the apparition in question.

The following sketch may serve to show the lines on which our own reasoning in the matter will proceed. We are making a census, which, so far, shows that in this country the proportion of sane persons, in good health and awake, who within the last ten years have had a visual hallucination representing some living person known to them, is about one in three hundred. Now, let us make a supposition far below the actual mark, and confine the number of the acquaintances of each of these hallucinated persons to five. Let us further suppose that one of these five persons does actually die in the course of the ten years. This seems fair, on the whole; for, though in some cases more than one may die within that time, in others none may die. According to this estimate, then, the chance that the death will take place within twelve hours of the apparition will be one in $365 \times 2 \times 10 \times 5$; that is, one in 36,500: in other words, only one out of every 36,500 of the hallucinated persons will, in the course of ten years, hit off the coincidence by chance. But since the hallucinated persons are only a three-hundredth of the whole population, this means that the proportion of the whole population who will by chance have an apparition of a person known to them within twelve hours of that person's death is only one in 10,950,000. Now, we ourselves have a large collection of such recent cases, resting on good first-hand testimony; but let us put the number far below the mark, and say thirty cases. If, then, these thirty coincidences are to be fairly attributed to chance, the population of the country will have to be 328,500,000. But we cannot suppose that our appeal for evidence has reached the whole population; and we shall be making a sober estimate, if we reckon that within the given time ten times as many cases must have occurred as those we happen to have encountered. This brings the necessary population up to 3,285,000,000; and the number will be further immensely increased if we take count of the fact that many of the coincidences are extremely close, that the times of the two events fall not only within twelve hours, but within one. Thus the theory that chance would account for the cases could only be justified if the population of the country were several hundred times what it actually is. The *reductio ad absurdum* seems tolerably complete.

The case of dreams is of course very different. We are most of us constantly dreaming. A very large number of 'odd coincidences' between dreams and external events is certain to occur by mere chance, and the cases are rare where the correspondence is of a kind which strongly suggests telepathic influences. Here, therefore, Professor Newcomb's estimate is far more applicable; and we have always felt that dreams, by themselves, could not be expected to afford conclusive proof of telepathy. This, however, does not seem a sufficient reason for ignoring them; since, if the fact of telepathic communication be otherwise established, they may throw light which we could ill afford to neglect, on the nature of the mental and cerebral processes involved.

As regards 'haunted houses,' we readily admit, and have expressly pointed out, the far greater uncertainty of the evidence as compared with the best telepathic cases. But even here we differ from Professor Newcomb in seeing a distinction between the experiences which we deem of some *prima facie* importance, and the experience which he supposes when a person, lying awake an hour after midnight, hears some sound the cause of which is beyond his power to guess. Sounds are the very weakest sort of evidence. What strength the *prima facie* case has, depends, not on things heard, but on things seen; and seen, not by one person only, but by several independently and at different times, and, as the seers affirm, without any knowledge, on their part, that the house was supposed to be 'haunted.'

Professor Newcomb's concluding remarks, dealing with the *experimental* side of telepathy, deserve careful attention. But his objections here rest entirely on the hypothesis of visual and auditory indications consciously or unconsciously given by the 'agent' to the 'percipient;' and though it is difficult, I know, to convince persons who have not been present that sufficient precautions have been taken to eliminate this source of error, it must surely be admitted that such precautions are possible. As regards sight, no one will deny the possibility; and, as regards hearing, we think, that, if a careful watch is kept, the means of communication resolve themselves into slight variations of breathing. Such variations were never detected in our experiments, and in any case could hardly be supposed capable of rapidly conveying to the percipient's mind the form of an irregular diagram; and the difficulty would be increased in cases where the signs would have had to be *unconscious*, as in many of our experiments where we were able not only to vary the 'agent,' but to act ourselves as 'agents.' As for 'indications whether the subject is going right or wrong,' they must, of course, be prevented by taking care that the 'agent' shall not watch what the 'percipient' is doing. Most of the spurious 'thought-reading' of the 'willing-game' would be prevented, if the 'willer,' instead of the 'willed,' were effectively blindfolded.

But we find ourselves once more wholly in sympathy with Professor Newcomb, when he insists that the experiments must be repeated again and again, under the strictest conditions, before we can reasonably expect thought-transference to be accepted as an established scientific fact. So far from resenting the demand for more evidence, we are ourselves unceasingly reiterating it. The responsibility for such novel observations cannot be too widely spread, and glad indeed shall we be to shift some of it to American shoulders. EDMUND GURNEY,

Hon. sec. of the Society for psychical research.

14 Dean's yard, Westminster, S.W.,
Nov. 4.

Mr. Gurney's letter suggests many interesting reflections on the probabilities involved in questions of telepathic phenomena, and I hope for an early opportunity to engage in a further discussion of the subject in the columns of *Science*. This will naturally involve the consideration of the points raised in his letter. Meanwhile there are two numerical data;

and, if he would favor me with them, I should feel much flattered, — firstly, his estimate, from the census results, of the number of persons of the age of fifteen and upwards, resident in the British Islands, whose statements he would consider *prima facie* entitled to full credence (to guide him I may remark that I see no reason why the number should not be from ten to twenty millions); secondly, his estimate of the probability that one of these persons, taken at random, would not be above amusing himself or herself at the expense of a society so eminent as that of which Mr. Gurney is the honorary and honored secretary. These numbers will come into my discussion, and I should much rather have them from an authority conversant with the subject than attempt to guess at them myself.

Simon Newcomb.

Change in the color of the eye.

The experience of Mr. T. F. McCurdy (p. 452) is, I imagine, a not uncommon one. It certainly finds illustration in my own family and in myself; the iris, which was quite black in childhood, having for many years visibly lightened, until it is more correctly described as gray, with shades of hazel. The fading-out of black eyes with age is a matter of common observation; and the change, judging from the facts within my own knowledge, is more apt to occur where the individual takes after a grandparent who had the dark eye, and where the immediate parents had blue or gray.

C. V. Riley.

Washington, D.C.

Specimens illustrating Lehmann's 'Origin of the crystalline schists.'

It may not be uninteresting to the geological readers of *Science* to know that the writer has recently received, through the kindness of Professor Johannes Lehmann of the University of Breslau, a very valuable suite of rock specimens illustrative of the latter's new and important work on the origin of the crystalline schists, noticed in *Science*, No. 86, p. 327, and in the *American journal of science* for November, 1884, p. 392. These specimens are sixty-three in number, and were collected, partly by Professor Lehmann himself, and partly under his immediate supervision, in the granulite area of Saxony, and in those parts of Bavaria which he has made the subject of his special study. They exhibit in an excellent manner nearly all those phenomena ascribed by the author of the above memoir to metamorphism by pressure, especially, however, the changes which certain massive pyroxene rocks of Saxony have undergone in becoming hornblendic schists exactly analogous to similar alterations traced by the present writer in the rocks near Baltimore.

To all students of metamorphism and of structural geology in highly crystalline regions, this work must be of absorbing interest as undoubtedly the most advanced of its kind; and, in spite of its superb atlas of most satisfactory photographic illustrations, its readers may be glad to know that this suite of original specimens is in the petrographical laboratory of the Johns Hopkins university, where it will always be accessible to such persons as may be interested in examining it.

Geo. H. Williams.

Baltimore, Nov. 25.

Bot-flies in a turtle.

Some days ago Prof. T. Robinson of Howard university called my attention to a box-turtle (Cistudo carolina) which had in the muscles on either side of the neck about thirteen large bot-fly larvae. The turtle was alive, but evidently suffered inconvenience from the intruders which had taken up their abode at a point from which they could not be dislodged by claw or beak. They were removed with forceps, and sent to Mr. Howard of the agricultural bureau, who informed me that they belonged to the family Oestridae, and to a genus probably undescribed. He also brought to my attention an exactly parallel case reported in the *American naturalist* (xvi. 598) about two years ago by Prof. A. S. Packard.

Frederick W. True.

U. S. national museum, Washington,
Nov. 24.

On the function of the serrated appendages of the throat of Amia.

Through the kindness of Prof. B. G. Wilder I have at present two living specimens of Amia which I propose to employ shortly in a comparative study of the brains of American ganoids.

My attention was first attracted to the serrated appendages of the throat by Professor Wilder's own note upon the subject, published in the *Proc. Amer. assoc.*, 1876, and more recently by a reference to the same structures in one of Sagemehl's admirable contributions to the anatomy of fishes (*Morph. jahrb.*, x. 63). Sagemehl concludes, from the examination of alcoholic specimens, that these 'flagella' are, during life, in constant motion, and thus help to renew the water in the gill-cavity. Such is by no means the case. The 'flagella' are attached by their bases to the lateral aspects of the sterno-hyoid muscles (hypopectorales of McMurrich), the chief function of which is to enlarge the cavity of the mouth. When these muscles are at rest, the flagella lie flat along their surfaces: when they contract, the cavity of the mouth is enlarged, the flagella erected, and the gill-covers pushed outwards. At the suggestion of my assistant, Mr. A. B. Macallum, we stimulated the proximal part of the muscle with the result of a perfect demonstration of the above facts. The flagella thus help to replace functionally the absent dilatator muscles of the gill-covers. A strip of condensed tissue occupies a precisely similar position on the hyopectoral muscle of Amiurus, perhaps a rudiment of similar organs possessed by the ancestors of the siluroids before the differentiation of the dilatator muscles of the operculum.

My specimens of Amia, after being in captivity for some time, became very sluggish, and hardly any movements of respiration could be detected. After the fish had been removed for a little out of the water, however, and then returned to it, the movements were sufficiently active to disclose the following facts: —

During the enlargement and filling of the cavity of the mouth, the posterior flexible (and muscular) border of the gill-cover is tightly applied to the soft parts behind the gill-opening. When the mouth-cavity is quite full, the mouth closes, the muscular border of the gill-cover releases its sucker-like hold of the soft parts, and the water is driven out by the contraction of the walls of the mouth-cavity.

Professor Wilder's account of the structure of the serrated appendages is so complete as to render any further reference to this subject unnecessary.

R. Ramsay Wright.

University college, Toronto,
Nov. 27.

Ergot nectar.

During the past summer I received a kind of grass from several of the northern states, which was sent me under the name of manna-grass. It proved to be Glyceria fuitans. It was stated that the bees were gathering large quantities of very delicious honey from this grass. I could readily believe the report, as the grass was covered with small crystals, as if it might have been wet, and dipped into granulated sugar. This sugar was very sweet and pleasant; and I have no doubt, that, like the nectar from Aphides, it would be wholesome winter food for bees, and no injury in honey for the market. The bees expressed the same opinion, as I learn that they would not leave this grass even for clover or linden bloom.

Upon examination, I found that the grass was covered with ergot grains, and that the nectar was a secretion from this poisonous fungus.

We see, then, that even the poisonous ergot, which I believe some of our best veterinary scholars think caused the so-called 'foot and mouth disease' among the cattle of Kansas last winter, has its wholesome uses. Why the ergot secretes this pleasant sweet, is hard to answer. The nectar, doubtless, serves the fungus in some way. A. J. COOK.

Agricultural college, Michigan,
Nov. 25.

THE 'OLD STONE MILL' AT NEWPORT.

FINDING myself in Newport lately, I took occasion to make some measurements upon that old circular building about whose origin (whether English or Norse) there has been so much dispute. I have not the slightest title to an opinion upon that subject, in which I have only a metrological concern. The building is circular, and rests upon eight cylindrical pillars. It is of such a size that any one would say, before measuring it, that the pillars would be circumscribed by a circle of four yards radius, and inscribed by one of three yards radius. The building could not have been erected without a drawing to scale, so that a unit of length must have been employed, and that unit (whether Norsemen or English were the builders) would undoubtedly be a foot. The Icelandic foot was, I take it, the same as Denmark and the Scandinavian countries used up to the adoption of the metric system; that is to say, it coincided with the Prussian foot of 12.36 inches English.

I found the diameters of the structure, measured at the pillars, as follows: —

	From outside to outside of the shafts.		Between the inward sides.
	24 feet 8 inches.		18 feet 6 inches.
	24 " 8 "		18 " 5 "
	24 " 9 "		18 " 4 "
	24 " 7 "		18 " 5 "
Mean . .	24 feet 8 inches.	Mean . .	18 feet 5 inches.

I think there can be little question that these lengths were meant to be 24 and 18 of the feet used. But supposing that I ought to have gone, say, farther out for the outer diameter (for instance, as far as the bases of the pillars extend), then I ought to have cut off the internal measure by the same amount; so that the mean of the two measures that I have taken is almost certainly 21 of the original feet. This mean is 21 feet 6½ inches, which, divided by 21, gives 12.31 inches as the length of the foot used. Besides the two lengths just mentioned, I found no other of sufficient magnitude, which I could conveniently measure, except the heights of the pillars. These appear to be intended to be 8 feet from the top of the base to the upper side of the cap-stones. The latter are 6 inches thick, as well as I could judge, leaving 7½ feet for the height between the base and capital. This could readily be measured with a tape-line, and was measured[1] on the insides of the pillars at two places on each pillar, — one at the right, and the other at the left. The following are the results: —

North arch.	East arch.	South arch.	West arch.
7 ft. 7 in.	7 ft. 8 in.	8 ft. 2 in.	7 ft. 7¼ in.
7 " 5 "	7 " 8¼ "	8 " 2 "	7 " 8 "
North-east arch.	**South-east arch.**	**South-west arch.**	**North-west arch.**
7 ft. 7¼ in.	7 ft. 9 in.	8 ft. 0¼ in.	7 ft. 6 in.
7 " 8¼ "	7 " 8 "	8 " 0 "	7 " 6¼ "
East arch.	South arch.	West arch.	North arch.

The mean of these is 7 feet 8⅞ inches; but the two south-west pillars are so different from the others, that I think it is more satisfactory to adopt the middling heights. Excluding, then, the two highest and two shortest pillars, the others measure

	7 feet 8 inches.
	7 " 8¼ "
	7 " 8½ "
	7 " 7¾ "
Mean . .	7 feet 8⅛ inches.

We have, then,

	Outer diameter.	Inner diameter.	Height.
Presumed intentional measure,	24 ft. 0 in.	18 ft. 0 in.	7 ft. 6 in.
Same in English feet, if foot used was 12.31 English inches,	24 " 7.4 "	18 " 5.6 "	7 " 8.2 "
Same, if foot used was the Scandinavian foot of 12.36 English inches,	24 " 8.6 "	18 " 6.5 "	7 " 8.7 "
Observed	24 " 8 "	18 " 5 "	7 " 8.1 " (7 " 8.9 ")

I made some other measures, which, though I think them of no value for determining the value of the foot, I proceed to give.

[1] The tape-line is believed to require about half an inch negative correction for all the measures. This has not been applied, as I have been unable to obtain the tape to verify the correction. In any case, such a correction is negligible in measuring so rough a structure.

Heights of the pillars on the outside.

	7 feet	3½	inches.
	7 "	2	"
	7 "	4	"
	7 "	6	"
	7 "	3	"
	7 "	10	"
	7 "	7	"
	7 "	5	"
Mean . .	7 feet	5$\frac{1}{16}$	inches.

Circumferences of the pillars.

	10 feet	1½	inches.
	10 "	2	"
	10 "	0	"
	9 "	9	"
	9 "	9½	"
Mean . .	9 feet	11.6	inches,

giving diameter, 3 feet 2.1 inches.

Sockets for jambs below.

Breadth . . .	0 ft. 4½ in.
Height . . .	0 " 4 "
Depth	0 " 2½ "

Original height of frame, 2 feet.

South-west niche.

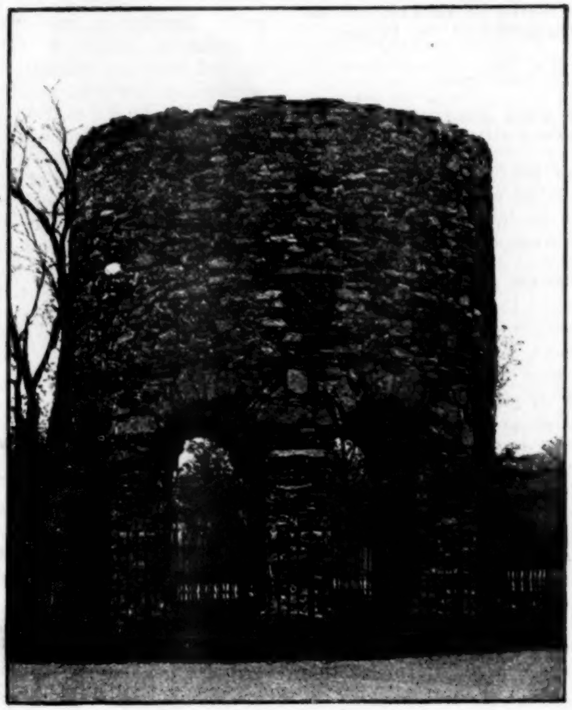
Breadth . . .	1 ft. 6 in.
Height	1 " 3 "

West window.

Breadth . . .	2 ft. 2 in.

Higher socket.

Breadth . . .	1 ft. 6 in.
Height	1 " 4 "



Diameters of bases of columns (most regular).

	3 feet	10	inches.
	3 "	9½	"
	3 "	9¾	"
	3 "	10	"
Mean . .	3 feet	9⅞	inches.

Circumferences of bases.

	12 feet	2	inches.
	11 "	9	"
	12 "	1	"
	12 "	2	"
Mean . .	12 feet	0½	inches,
corresponding to diameter,	3 "	10	"

Fireplace.

Breadth . . .	3 ft. 5 in.
Height	4 " 0 "
Breadth at base,	2 " 7 "

Niche on the right of fire-place.

Breadth . . .	2 ft. 3½ in.
Height . . .	2 " 1½ "

Small niche or socket on south side.

Breadth . . .	1 ft. 6½ in.
Height . . .	1 " 8 "

South window.

Height outside,	2 ft. 5½ in.
Breadth . . .	2 " 2 "

My ladder was too short to enable me to measure the upper parts.

Without wishing to express any archeological opinion whatever, I cannot refrain from saying, that, as far as I could perceive, all the rough-cast covering the walls, the smooth mortar in the sockets, etc., were a part of the original mortar. There is a certain amount of later mortar, but it is readily distinguished upon close inspection.

The fireplace has two flues; and the windows formerly had frames, as if for holding glass. The projections of the pillars beyond the upper part of the tower suggest that there might have been a ledge upon which a miller could climb to turn round the axis of the sails of

the wind-mill. The two separate flues to the fireplace might prevent the draught from being interfered with by the axle. But would not a fire in a grist-mill be dangerous?

The hearth of the fireplace was elevated above the floor, as in a forge. The building had two stories above the ground. Its total height is about twenty-five feet.

The stones, many of them granite, show no drill-marks and no marks of an axe, but do show marks of the hammer. C. S. Peirce.

THE 'HOOD' OF THE HOODED SEAL,[1] CYSTOPHORA CRISTATA.

All the figures of the hooded seal which I have seen represent the animal with a great bunch on the top of its head. This bunch is made to vary somewhat in shape, size, and position, in the different illustrations; but all agree in placing it on top of the head, no part ever protruding beyond the jaw. It is sometimes pictured as extending transversely across the crown, sometimes as a double or single roll reaching from the nose to the occiput. The earliest delineation of it which has fallen under my observation is that given by the old missionary, Hans Egede, in his description of Greenland, published in 1741 (fig. 1). Crantz, who was also for many years a missionary in Greenland, said, "The forehead is furnished with a thick folded skin, which the animal can draw over its eyes like a cap, to protect them from stones or sand driven about by the surf in a storm." And even Dr. Rink, in his recent excellent work on Danish Greenland, says that this seal 'is well known from the bladder on its forehead.' In Griffith's

Fig. 1.

'Cuvier' it is stated that the hooded seal "has the power of bringing a fold of skin placed on the forehead, forward, so as to cover the eyes, which it does when threatened, or about to be struck. . . . When at rest, or drawn back, it considerably enlarges the apparent size of the neck and shoulders." The only adult hooded seal, so far as I am aware, possessed by any museum in America, is in the American museum of natural history at Central Park, New York. Its head is very well represented in the accompanying drawing (fig. 2).

Fig. 2.

Determined to visit the seal-fishery in person, I set sail from Halifax in February, 1883, proceeding northward from Newfoundland in the cabin of the ill-fated Proteus, in her annual cruise to the sealing-grounds. On the 18th of March, after a somewhat laborious walk over an ice-floe, I found myself face to face with a family of hoods, and discovered that the male, — a huge beast, bigger than an ox, — instead of having a crest, or fold of skin, on the top of his head, was provided with a great proboscis, suggesting that of the sea-elephant of the antarctic (fig. 3). He looked on with apparent indifference, while his mate, solicitous for her young, advanced to meet me, growling fiercely, and displaying her sharp, curved teeth. Wishing to observe her actions, I annoyed her for a few minutes with my gaff, — a proceeding which it is by no means safe to undertake with the male. While this encounter, in which she was the aggressive party, was in progress, her spouse began to manifest symptoms of uneasiness, and finally became very much enraged, though he did not attempt to drag his ponderous body to the scene of the conflict. He at first showed his displeasure by frowning, and wrinkling the skin on his long snout. The tip of the proboscis was then inflated and emptied several times in rapid succession, after which the entire 'hood' was partially inflated. In

[1] Abstract of a paper read before the Biological section of the American association, Sept. 9, 1884.

addition to its numerous and ever-changing contractions, there was one rather constant constriction about opposite the nostrils, incompletely dividing it transversely into two portions, the anterior of which, though dark in color, much resembles a bladder, and explains the vulgar epithet, 'bladder-nose,' often applied to this species. A curious fact observed was, that, during the alternate filling and emptying of the sac, a noise was produced which closely resembled that of bubbles of air rushing into a bottle from which a liquid is being poured. It was a loud, gurgling sound, audible at a distance of twenty-five metres or upwards. On approaching nearer, the animal became furious. He inflated his 'hood' to such an extent that all traces of constriction were obliterated, and, by a series of ugly tosses of the head, kept it swinging from side to side.

During the ten days that followed, about

FIG. 3.

fifteen thousand seals were killed and hauled aboard by the crew of the Proteus; and I had ample opportunities for observing their actions both upon the ice and in the water. I therefore state, without fear of contradiction, that it is utterly impossible for the animal to arrange his head-gear in the manner shown in fig. 2, or, for that matter, in any figure that I have seen.

The largest males which I killed measured ten feet in total length (from tip of nose to end of hind-flipper), and eight feet in girth. I think that they do not attain their full growth until ten or twelve years of age. In the largest individual measured the uninflated proboscis extended two hundred and twenty-five millimetres (nearly nine inches) in front of the upper lip. The height of the proboscis midway between the nostrils and tip was two hundred and thirty millimetres; height at mouth, three hundred and twenty millimetres. This curious development is purely a sexual character, no trace of it existing in the female. It begins to appear in the third year, when, by passing the fingers into the nostrils, it may be detected as a small sac at the extreme end of the nose, divided longitudinally by the nasal

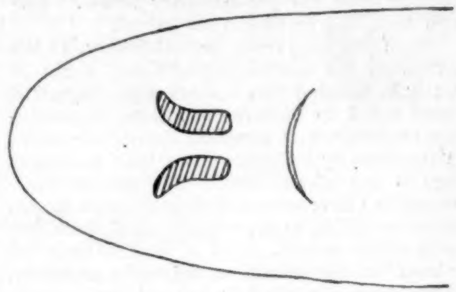


FIG. 4.

septum into two distinct chambers, which remain distinct throughout the animal's life. So far as I was able to ascertain from the examination of a very large number of individuals, it continues to grow for ten or twelve years.

Dissection of the proboscis, when in the adult condition, shows it to be a loose muscular bag, covered with the skin of the nose, and lined with a continuation of the nasal mucous membrane. It is completely divided for its entire length into two parallel chambers by a thin partition, which consists chiefly of two layers of mucous membrane, and is continuous with the nasal septum. The nostrils (fig. 4) are capable of closure by the contraction of muscular fibres, which are so arranged as to act as sphincters. To prevent interference in breathing by the falling together of the walls of this

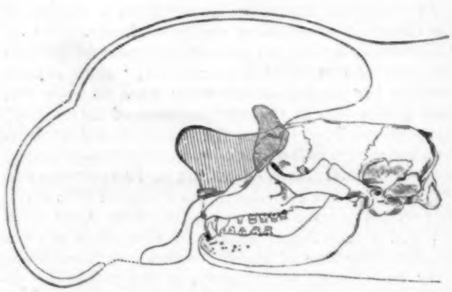


FIG. 5.

redundant bag, the roof of the proboscis is supported by three large and stout cartilages, — one median and two lateral (fig. 5). The median or septal cartilage, which is a continu-

ation of the mesethmoid, rises above the plane of the top of the skull, and extends forward beyond the jaw. A bilateral expansion of its base in front forms a firm supporting pad, resting upon the pre-maxillary bones. The two remaining cartilages are paired.

Since returning from the seal-fishery, I have examined the accessible works that would be likely to mention this curious appendage, but have failed to discover any thing approaching an accurate or complete description of it. That given by Fabricius more than a century ago is one of the best All writers whose accounts I have seen, including the most recent, agree in failing to express the chief characteristic of the animal, which is, that the so-called 'hood' of the male is an inflatable proboscis, protruding considerably beyond the mouth, which it overhangs.

C. Hart Merriam, M.D.

MEASURING EARTHQUAKES.[1]

In view of the recent earthquake in England, and the still more recent shakings which parts of this country have experienced, a notice of the above work will be of especial interest. Professor Ewing's long residence in Japan as professor of mechanical engineering in the University of Tokio, and his active labors in connection with the Seismological society there, of which he was vice-president, entitle him to speak with authority on this subject. Indeed, in this matter of the exact measurement of the motion of the ground during an earthquake, seismologists the world over must look for enlightenment to young Japan, whose Seismological society, under the guidance of the foreign professors in her university and her college of engineering, has in this particular branch far outstripped European seismologists.

In chapter i. Professor Ewing gives a *résumé* of the theory of waves in an elastic solid, as applied by Hopkins, in the British association report for 1847, to the case of terrestrial disturbances; "since it both teaches the earthquake-observer what to look for, and guides him in the interpretation of his results." This shows how, from a single sudden disturbance, two series of waves will set out in all directions,— the first or normal waves consisting of compression and expansion of the material in the direction of transit; the second or transverse waves travelling more slowly, and consisting of motion of distortion at right angles to the line of transit, — also how these waves may be reflected or refracted at the bounding-surfaces between different strata, and thus by successive reflections be reduplicated; so that, at a distant point, the vibrations will probably be far different from (in number, order, phase, and period), and generally much more complicated than, those at the origin. Add to this the effect of imperfect elasticity, and the condition that the original disturbance may be a series of slips along a whole line or 'fault,' and nothing further could be desired to give confusion to the vibrations.

Chapters ii. and iii. deal with instruments for measuring the horizontal motion of the ground. At the outset Professor Ewing notes the difficulties in the way of getting a steady point, or something 'to tie to,' while every thing around is being shaken; and the characteristic feature of every seismometer is its method of supporting a heavy mass so that it will remain steady, receiving no impulse (save what is unavoidable through friction) while the system that supports it is being shaken. As the 'horizontal pendulum' seismograph in one of its forms is considered the best, and has given the greater part of the records obtained, its essential feature is here shown in fig. 1. Popularly it might be termed a heavy weight, swinging on a gate. It is a heavy mass, pivoted upon a vertical axis through *d*, upon a frame free to move about the vertical axis *bc*. The long light reed multiplies the motion, and records it upon a rotating smoked-glass plate by the steel pointer on its end. This reed is pivoted at *d*, with most of its weight taken up by the coil-spring, whose tension is adjustable at *e*. The parts of this supporting lever and long reed are so proportioned that in the vertical axis through *d* lies the centre of percussion relative to the axis *bc*: hence, if this is shaken through *bc* at right

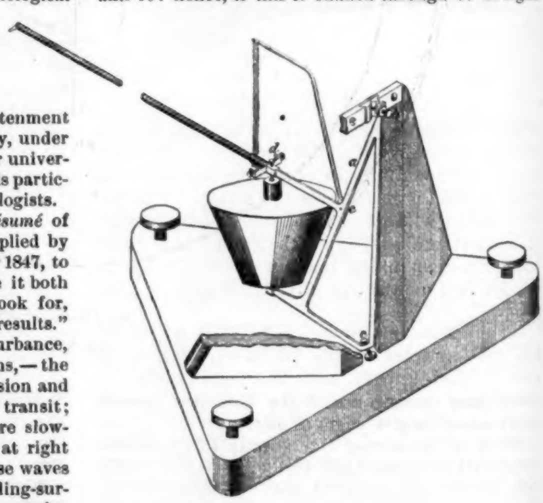


Fig. 1.

angles to the plane of the lever, the vertical through *d* will be of itself the axis of instantaneous rotation, independent of the heavy mass pivoted there; hence the latter will receive no impulse at right angles to

[1] *Memoirs of the science department, Tôkiô Daigaku (University of Tôkiô), No. 9. Earthquake measurement.* By J. A. Ewing, B.Sc., F.R.S.E. Tôkiô, *Tôkiô Daigaku*, 1883. 12+92 p., 23 large plates. 4°.

the plane of the lever, save that due to the very slight friction at b and c and at the marking-point; and the purpose of the heavy mass is chiefly to furnish, by its inertia, the necessary fulcrum upon which to overcome this slight friction. These principles, as regards centre of percussion, and axis of instantaneous rotation relative to the axis of support, are especially insisted on by Professor Ewing as essential features of a reliable seismometer. Two of these horizontal pendulums at right angles record the two rectangular components of the horizontal motion upon a rotating smoked-glass plate.

Many other ingenious and novel forms of apparatus for registering the horizontal motion of the earth are described in chapters ii. and iii., and various devices are described in chapter iv. for measuring the vertical component of the motion.

We reproduce the trace of the earthquake of 1881, March 8. Fortunately, Professor Ewing was present during its occurrence, and, as it drew to a close, lifted the marking-levers from the plate, so that their subsequent trace should not obliterate at all the record already obtained. Fig. 2 shows this record, made by a pair of horizontal pendulums multiplying the motion six times. As here reproduced, it is about twice the actual motion of the ground. The inner circle gives the N-S, and the outer the E-W components. At d and d' are two cross marks, showing where the pointers rested when the plate was stopped; and their angular distance is that to be used in connecting the two circles to obtain the simultaneous motion of the two points. The motion began on the outer, E-W, circle at a (corresponding to a' on the inner), and thence can be easily traced nearly twice round the circle to c, where the pointer was lifted (corresponding to c' on the inner circle). There was no appreciable motion in a N-S direction till about ten seconds after it began at a, E-W; but it began quite suddenly just before b', and can be traced twice round to where the plate stopped at d'. The rotation time was about eighty seconds, and the earthquake had lasted about two minutes and a half when the plate was stopped. Feeble movements were observed some time longer.

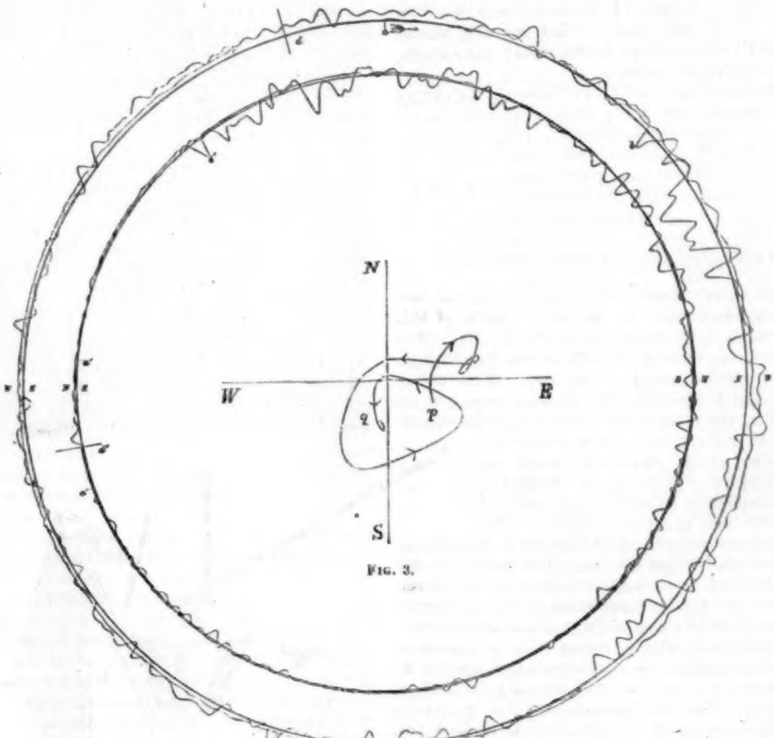


FIG. 3.

FIG. 2.

In fig. 3, Professor Ewing has carefully combined the motion of the two components so as to give the actual path of a point of the earth's surface for a short interval. From p to q, following the arrow-heads, it shows that motion (magnified six times) during an interval of three seconds.

This same characteristic wriggling motion is shown in fig. 4, which is the entire record, magnified three and a half times, upon a stationary plate, of an earthquake which occurred on April 23, 1883, and lasted four minutes and a half. It is quite likely that the earthquakes which recently shook up our middle and western states would have given a somewhat similar record.

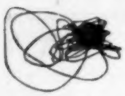


FIG. 4.

The principal characteristics of the average earthquake in the plain of Tokio are: 1°. The motion of the ground begins very gradually. 2°. An earthquake consists of many successive movements, and there is almost always no single large one which stands out prominently from the rest. 3°. The disturbance ends even more gradually than it begins. 4°. The range, the period, and the direction of movement, are exceedingly and irregularly variable during any one earthquake. 5°. The duration of disturbance of the ground is rarely less than one minute, and often several minutes. 6°. Even in somewhat destructive earthquakes, the greatest displacement of a point on the surface of the soil is only a few millimetres. 7°. The vertical motion is generally much less than the horizontal. 8°. A mass shaken back and forth in the most severe earthquakes of Tokio, would, if it did not slide, be urged by a horizontal force, which, at its maximum, would equal about one thirty-third of its weight. This, regularly repeated, is sufficient to crack brick walls, and sometimes throw down chimneys.

To the many readers who have had no experience of earthquakes, but are accustomed to think of them as a sudden violent thrust, accomplishing at a blow, as it were, all their disastrous work, the preceding descriptions will be a somewhat new revelation.

Professor Ewing plainly shows that as seismometers, the instruments in use by Palmieri and others are worthless; for not one of them can be depended upon to give a reliable measure of the direction, period, or amplitude of the vibrations of the ground, most of them being designed to record a single violent thrust in one direction, and nothing subsequent, and the greater part of them being some form of stable pendulum which is almost certain to be set swinging in an earthquake through amplitudes far greater than the earth itself, and thus to mask entirely the motion of the latter.

One novel 'time-taker,' the invention of Professor Milne, whose work in Japan is so well known, is worthy of note. A clock has its hour, minute, and seconds hands all on the centre of the face, and of different lengths, with their ends turned up into the same plane, and tipped with cork smeared with printer's ink. In front is a track upon which, when a seismoscope closes a circuit, a carriage travels up and presents a disk to the face, and then backs off again, carrying an impression of the instantaneous position of the three hands, and leaving the clock to go on undisturbed.

The closing chapter treats of the constructive details and requirements of a seismological observatory; and a series of experiments by Professors Milne and Gray are noticed, in which it was sought to determine, by a series of artificial earthquakes (dropping heavy weights in a foundery, and exploding buried cartridges of dynamite) in connection with time-recording seismometers, the velocity of transmission through the ground. These gave 438 feet per second for normal, and 357 feet per second for transverse, waves. This was through hardened mud. Mallet's earlier experiments gave for sand 825 feet, for jointed granite 1,306 feet, and for solid granite 1,665 feet, per second. The last, Professor Ewing remarks, is probably very much too low.

This element of earthquake motion, the velocity of transmission through the earth's crust, is very inexactly known; and the author notes the desirability of extending the observation of earthquakes over a considerable region of such a country as Japan by means of many stations connected by telegraph, to which simultaneous time-signals can be sent, and at which the same earthquake may be recorded on rotating plates, together with a record of the absolute time. These, if sufficiently widely distributed and numerous enough, would give us valuable data regarding the latitude, longitude, depth, and time of the origin of the disturbance, and the velocity of its transmission to the surface in all directions, supposing it rectilinear and uniform. Regarding the possibility of this, Professor Ewing, in the article referred to above (*Nature*, June 19, 1884), speaks as follows: "But all this depends upon our being able to recognize at the various stations, some one wave out of the complex records deposited at each; and, especially in view of the curvilinear nature of the motion, it would be hazardous to say, without trial, whether this can be done."

In conclusion it may be said, that the whole work is exceedingly interesting and valuable; and Professor Ewing is to be highly commended for thus bringing together the best results of modern methods in exact seismometry, and for showing the sources of error and the fallacies in older methods and theories. The work should receive as wide a distribution as possible by the University of Tokio. H. M. PAUL.

EXCURSION MAP OF THE VICINITY OF BALTIMORE.

THE need has long been felt, among those students of the Johns Hopkins university who are especially interested in the study of natural history, of a reliable map of the adjoining country, on a suitable scale, and so mounted as to be adapted for convenient pocket use. It is believed that a few words regarding the method by which the want of it has been recently

supplied in Baltimore, will have more than a merely local interest.

The students and instructors of the university whose work naturally inclines them to out-of-door explorations are associated in what is known as the Naturalists' field-club; and it was decided by the members of this club, that a map on a scale of a mile to the inch, covering an area twenty-five miles square, with the city-hall of Baltimore as its central point, would amply supply all present needs. The difficulties in the way of the construction of such a map were, however, considerable. The cartographic materials already existing were very fragmental, and varied much in their form, scale, and reliability, while the great cost of a new survey of so large an area was out of the question. Mr. Albert L. Webster, however, who had had four years' experience as topographer on the U. S. geological survey, submitted a plan to the trustees of the university, which met with their hearty approval.

All maps, of whatsoever kind, relating to the area in question, were collected and carefully compared, the most accurate of them being reduced to a uniform scale. A drawing on the scale of two inches to a mile was then commenced, upon which, however, only the most reliable material — the work of the U. S. coast-survey, which covered about one-third of the entire area — was incorporated. The remainder of this drawing was left blank, with the intention of placing upon it in future only such material as is up to the standard of the coast-survey work. For the remaining two-thirds of the area a tracing was made from the best existing sources, and the two together (drawing and tracing) reduced one-half, and photolithographed. The present published map, therefore, is on a scale of one mile to an inch, and represents the best existing information relating to the vicinity of Baltimore. It is, however, not in any way to be regarded as complete or final, but only as the first step toward the attainment of a really good representation of the region. It is doubtless faulty in many particulars, and is certainly very deficient in showing no topography. With a view to its improvement, any suggestions relating to either details or the general character of the map, as well as any information regarding accurately determined elevations within the area, are earnestly solicited from all persons who may make use of it. In this way it is hoped that the map may be a constant growth, improving year by year through the criticism and suggestions of those interested in it.

After the original drawing has once been made, the cost of embodying improvements and publishing successive editions is not large, and may easily be defrayed by the sale, at a moderate price, of the printed copies. The Baltimore maps, cut into sections and mounted on linen, folding into a pocket-case, are sold at a dollar each.

These details are given in the hope of eliciting suggestions, or of inciting similar clubs, in other cities where a good map is as much needed as in Baltimore, to start the development of something of the same kind.

THE NEW-YORK AGRICULTURAL STATION.

Second annual report of the board of control of the New-York agricultural experiment-station for the year 1883, with the reports of the director and officers. Albany, *Weed, Parsons, & Co., pr.*, 1884. 279 p. 8°.

In the space at our command it is impossible to make any adequate review of the large amount of valuable work which we find in the New-York report. In general it may be said that it partakes of the characters of both the classes of experiments spoken of in our comments on p. 509. Some of it lies on the border-land between the two, yielding results of more or less immediate value to both science and practice. We include here such experiments as those upon methods of cutting seed-potatoes; the influence of depth, and distance apart, of planting, upon the crop; the effects of mulching, cultivation, root-pruning, and the like. Others are more distinctively scientific in their aim, such as the lysimeter observations, the notes on hybridization in maize, the experiments upon the influence of food upon milk and butter production, etc.

Perhaps the most noteworthy portion of the report is its proposed method of classification of artificial varieties of plants for purposes of identification. This method is based on the belief, confirmed by two years' observations, that those portions of the plant for whose sake it is especially cultivated are comparatively constant in form within the same variety, under the circumstances of cultivation, while the agriculturally unimportant parts may show considerable variations. For example: the roots of any particular variety of beet will show comparatively little variation, while the tops may present very considerable differences. Artificial selection has here impressed certain desired qualities upon the root, but paid little or no attention to the tops.

Proceeding upon this belief, it is proposed to base the classification in 'agricultural botany' upon the agriculturally important part of the plant. Thus all root-crops would be united into one class, irrespective of their ordinary botanical relationships, this class to be subdivided into smaller groups in accordance with the form of the root.

Such a method of classification for a particular purpose would appear to be legitimate. Its final justification is to be sought in its success, and of this it is too early to judge. When the observations shall have been extended over a term of years, and the constancy

of varieties established, agricultural botany may prove of much value to the farmer, gardener, and seedsman. Until then it belongs in the category of hopeful experiments.

MINOR BOOK NOTICES.

A treatise on the adjustment of observations, with applications to geodetic work and other measures of precision. By T. W. WRIGHT, B.A., late assistant engineer U. S. lake-survey. New York, *Van Nostrand*, 1884. 437 p. 8°.

THE student of the method of least squares often fails to grasp the true meaning and significance of the method, from the want of illustration and well-chosen applications. The chief merit of Mr. Wright's book is in the collection of examples which have been drawn from the records of actual work in which the author has been engaged. Besides the application of the methods of least squares to the results of triangulation and of levelling, a chapter is devoted to these methods in relation to line-measures in general, and to the calibration of thermometers.

There are some observers who are tempted to believe in the infallibility of certain criteria proposed by different writers for the determination of the weight of observations. There are others who reject the mathematical criteria, and prefer graphical methods as guides to a correct judgment. Mr. Wright is one of those who prefer to look at observations from the practical observer's point of view. His treatise will therefore be of interest to the mathematician who desires to frame criteria which will represent more closely the results of experience, and will prove of great utility to the practical man.

Recent progress in dynamo-electric machines, being a supplement to dynamo-electric machinery. By Prof. SYLVANUS P. THOMPSON. New York, *Van Nostrand*, 1884. (Van Nostrand sc. ser., No. 75.) 113 p., illustr. 24°.

THE writers who rapidly assimilate the advances in electrical engineering, and present their knowledge to the public in an intelligible way, are doing very useful work. The treatises of Professor Thompson are increasing upon the electrician's book-shelf. The time has not arrived for a standard treatise on electrical engineering, on account of the rapid changes and development of the subject. Until we can have such a standard treatise, we must rely upon brochures like this latest production of Professor Thompson.

The reader will find in it an account of Mr. Hopkinson's modification of the Edison dynamo, and also a description of the latest modifications of the Gülcher machine, and also of the Thomson-Ferranti machine.

Wonders and curiosities of the railway; or, Stories of the locomotive in every land. By WILLIAM SLOANE KENNEDY. Chicago, *Griggs*, 1884. 16+254 p. 12°.

ONE is a little startled, on opening this book, to find mentioned the "huge, ample-shadowed foundry; the peculiar fragrance of burnt earth and iron; . . . the boy controlling the huge steam-hammer; . . . and, finally, the great crane that lifts up the monster in chains, and carries it to the doorway, and sets it down in all the resplendence of its polish and paint, ready to begin its thirty years of toil," with nothing predicated of them; but is relieved immediately by the statement that 'this is the building of the locomotive.' This introductory chapter, in which 'our old Homeric poet Whitman' receives praise, and which may have been written by him, should not, however, deter the reader from going deeper into the book. From chapter ii. on, the writer tells the anecdotes he has collected in regard to the railway, and has succeeded in bringing together a most entertaining collection. The account given of the Quincy railway must change the impression that many have of that so-called 'first American railroad.' The chapter on the 'locomotive in slippers' is devoted to the history of the railway in the east, and at times is especially amusing. The author also touches upon the 'vertical railway' (the elevator), upon the various mountain railways, and upon the recent attempts to use electricity as a transmitter of power.

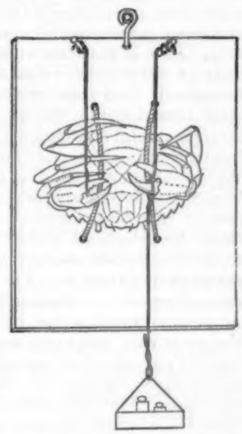
NOTES AND NEWS.

A CONFERENCE to formulate plans for the systematic observation and discussion of earthquakes was recently held in the rooms of the U. S. geological survey in Washington, at which there were present Messrs. Powell, Dutton, and Gilbert, of the survey, Abbé and Marvin of the signal-service, Paul of the naval observatory, Rockwood of Princeton, and Davis of Harvard college. It was decided that three classes of observations should be attempted; the first class consisting of those made by self-registering seismometers of approved pattern, upon which Messrs. Paul, Rockwood, and Marvin are to report at an early date. The second-class observations will be chiefly to determine the time of shock, probably by means of a

seismograph of relatively moderate cost. The third class will include ordinary non-instrumental observations according to a system to be prepared by Professor Rockwood. It is expected that a considerable number of the first and second class instruments will be maintained by co-operation of public institutions and government bureaus, at observatories, physical laboratories, army arsenals and signal-service stations, and navy-yards; while the instructions for the general observations will be sent to all the regular and volunteer observers of the signal-service, the members of the state weather services, and to all others who desire to aid in the work. In order to concentrate work on the most profitable districts, a chart of recorded shocks will be prepared by Professor Rockwood; and the selection of stations will then be discussed by Messrs. Rockwood, Abbe, and Davis. Further studies will be undertaken on the matters of bibliography, previous observations, and instruments; the whole work being under the direction of the geological survey.

—The recent works of the U.S. geological survey, and especially the remarkable report of Capt. Dutton, have given an opportunity to Professor Trautschold of Moscow to draw a parallel between the geological structure of Colorado and that of European Russia, which appears in the bulletin of the Moscow society of naturalists. In Russia the Silurian, Devonian, carboniferous limestone, and lower Permian series are marine deposits, while the upper Permian is of fresh-water or terrestrial origin. The trias and lower Jurassic rocks are also continental deposits, or seem to be so to a great extent, while the upper Jurassic groups are again of marine origin, as is also the chalk, which contains only islands with land vegetation. Three parts of the tertiary series consist of terrestrial and fresh-water deposits, marine deposits appearing only in the south; and the quaternary is also a continental formation. Such being, according to Professor Trautschold, the structure of Russia, he had already concluded that in the northern hemisphere there was a general retreat of the sea during paleozoic times, and a growth of continents, upon which the carboniferous and then the Permian floras largely increased; European Russia being, during the triassic and the first half of the Jurassic periods, a continent with nearly the same outlines as now. During the second half of the Jurassic period, another subsidence of the continent, and an advance by it into the northern hemisphere, again took place; without reaching, however, the same level that it had had during the paleozoic period, the sea remaining shallow. A second retreat of the water took place during the tertiary and quaternary periods. Similar oscillations might well explain, in Professor Trautschold's opinion, the structure of the Grand Cañon district, where the connection between the Jurassic and triassic is as close as in Russia.

—From recent experiments to determine the absolute force of the flexor muscles of decapod crustaceans, Professor Felix Plateau of Gand, Belgium, concludes that the absolute or static force of the muscles of the claws of crabs is relatively weak, and that while the adductor muscles of some lamellibranchs are comparable with those of mammals, and others with the more powerful muscles of the frog, the muscles of the claws of crustaceans can be compared only with the weakest muscles of the frog. The relation between the absolute force of the muscles of man and the greatest power Plateau had observed in the crustaceans, convinced him that the contractile force of the muscular fibre is not the same in all animals. Arthropods are inferior in this respect to mammals and to lamellibranchs. Crustaceans, like insects, have in proportion to their weight much greater power than the vertebrates. When experimented on, as in the figure given, he found that common crabs could raise from one to two and a half kilograms, representing a force which he thought clearly explained the mishaps undergone by these animals.



—The women medical students of Paris have presented a petition to the authorities for permission to walk the hospitals, and become house-surgeons therein. The petition is supported by a considerable number of physicians and surgeons.

—The ship Occidental, at San Francisco, Nov. 14, reports, "At six P.M., Nov. 4, a hundred and fifty miles off Mendocino, Cal., had three shocks of earthquake, and a few hours later two more heavy ones."

—Mr. E. Knipping, meteorologist of the Imperial meteorological observatory at Tokio, describes in the September number of the *Mittheilungen der deutschen gesellschaft für natur- und völkerkunde Ostasiens*, the rapid development of weather telegraphy in Japan. There are now twenty-four stations in the empire connected by telegraph; and on the basis of their observations, supplemented by despatches from China, three daily synoptical maps are published in Japanese and English characters. Observations are taken at six A.M., and two and nine P.M., 'Japan' time, which is about that of the Kioto meridian; so that the evening observation corresponds to eight o'clock 'China coast' time, six o'clock 'Bengal' time, four o'clock 'Persian' time, one o'clock 'German' time, and noon in 'English' (Greenwich) time. The director of the service is Mr. I. Arai; and the observers, telegraphers, draughtsmen, and printers are all Japanese. The first weather-map was printed on March 1, 1883, and the tri-daily issue began a month later. The chief

need of the service at present is the addition of the fifty-six lighthouses to the other stations, and the construction of a submarine cable to the Liukiu (Loo Choo) Islands.

— The first part of the Atlas of the western-middle anthracite coal-field has lately been issued by the Second geological survey of Pennsylvania, the work being in charge of Mr. C. A. Ashburner. It comprises the district between Ashland and Mahanoy City, and is in the same style of construction as the atlas of the Panther-creek basin, of which mention has already been made in *Science* (i. 309). The new maps fully maintain the high standard of accuracy, and the careful distinction between observation and inference that characterized the earlier number of this important contribution to practical geology. The atlas includes four mine-sheets (1:9,600, with underground fifty-foot contours of mammoth coal-bed), three topographical sheets (1:19,200, with surface form in ten-foot contours), and four cross-section sheets (scale, 1:4,800). The reference-lines, marking out squares of two thousand feet on a side, are now properly adjusted to the true meridian, instead of to the local and temporary magnetic north, as before; and the ground-colors representing the geological subdivisions are changed to tints of rather more agreeable tone. The full indication of the known facts on the basis of which the area and altitude of the coal-beds are represented, and their careful separation from hypothetical lines of outcrop and dip, make it possible for both the practical and the theoretical geologist to use these sheets with as little effect as possible from the personal equations of those who made the maps. Besides being issued, folded in the octavo atlas, the sheets can be bought, unfolded and singly, at simple cost of printing, — about fifteen cents apiece.

— In a recent lecture upon the languages of the American aborigines before the Lowell institute in Boston, Prof. D. G. Brinton endeavored to show the general characteristics of the American languages to be *synthesis*, or the blending of a number of words into one; *incorporation*, or the absorption by the verb of both subject and object; and the peculiar use of pronouns. Other features were described and illustrated, such as the absence of grammatical gender and of the true substantive verb, the rarity of numerals and of the true adjective, and the difference in the speech of the two sexes and of different ages and classes. In spite, however, of the absence of all etymology, these languages are very interesting. While they lack in parts of speech, they are rich in themes and ideas. They were shown to compare favorably with European languages in respect to their vocabularies and their ability to express abstract ideas, but to be deficient in respect to sentence-building. The lecturer claimed, however, that the importance of any language depended upon the use that was made of it. After showing that unwritten language is not necessarily liable to the greatest changes and fluctuations, and that language forms a satisfactory basis for studying the laws of ethnology, the characteristic features of the principal aboriginal tribes of North America were briefly sketched, and the peculiarities of their language described. The Narwatal language of Mexico was asserted to be the only aboriginal American language for which a regular professorship had been established in any university. The literature which survives, of the native languages of Mexico and Central America, was described. The lecturer closed by urging those who wished to study the American languages to do so at once, as the time was not far distant when these languages would have entirely disappeared.

— Professor Liversidge of the Sydney university, says *Nature*, sends to the local press a suggestive communication, in connection with the recent meeting of the British association in Montreal and the invitation forwarded by the Victorian premier to visit Melbourne next year. Feeling how insurmountable, for the present, are the obstacles to such a visit, the writer proposes what appears to be a very wise alternative. Instead of looking forward to a near visit from the association, he suggests as a preliminary step a federation of the various scientific societies in Australia, Tasmania, and New Zealand, into an Australasian association for the advancement of science, on the lines of the British association. A first meeting of the new association might be held in Sydney on the hundredth anniversary of the colony; which, with the combined attractions of an international exhibition, might induce a fair number of scientific visitors from England to take part in the proceedings. After the first meeting, gatherings could take place annually, or every two or three years, as might be agreed upon by the members, in various parts of Australasia. The writer concludes with the remark, which few will gainsay, that such an association would tend greatly to advance the sciences in the colonies, and in many ways materially favor their progress elsewhere.

— Dr. Kollmann announces a law of correlation governing the form of the face of European man. Two modern Swiss skulls from the collection at Basle, which may be duplicated in any collection of European crania, represent two types existing in the present population of Europe, — the broad-faced (chamaeprosope of the craniologist), and the narrow-faced (leptoprosope of craniology). The broad-faced variety is wide between the eyes, with broad low orbits, short nose with low bridge, wide nostrils, and broad mouth. The narrow-faced variety has slender features, round open orbits with eyes set near together, long nose with high bridge, narrow nostrils, and small mouth. Either variety, if pure, will present its characteristic features, while, if crossed, the degree of mixture may be determined by the number of features varying, and the amount of variation from the general type.

— Professor Haynes requests us to state that the closing sentence of his letter on p. 469 should read, —
"There is no doubt whatsoever that it is the relics of men very like those first found by Europeans on this continent, which Mr. Jacob Messikommer will help any one, as he did the writer, to disinter from the peat-moor of Robenhausen."